MISSOURI BOTANICAL GARDEN, ENGELMANN HERBARIUM

Rec'd July 9th
Ans - 91

Germantown Pa
July 4th 1879

Dr George Engelmann

Dear Sir

In writing to
Dr. Asa Gray recently, I
took occasion to send him
a flowering specimen
of what is known in
collections as Cereus
triangularis, with the
observation that it
appeared to me to be
an entirely new
genus. In a letter

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just received from him
he says "I hope you will
send the next specimen
you have to
Dr Geo Engelmann; he
will I know be pleased
and edified. From Dr
E's remarks I think he
would have considered
it as constituting a
new genus. If he had
seen sure the flowers
were terminal, which
from your specimen
they appear to be. 4

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I do not think I shall
have another specimen this
season, and to take the
liberty of calling your
attention to the plant
as it is. My plant
produced only four buds
this season — two of which
dropped before expanding.
The two others perfected —
One of them I sent to
Dr Gray, and the other I
left on in order to
have an opportunity
of studying the dead very

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Which is now smelling.
It differs from Cereus
in having scarcely any
tube, and that tube sud-
-denly abbreviated, or
contracted towards the
base. In fact there is nothing
in common with Cereus
in the flower, but rather
with Opuntia, with which
it agrees in the property
of opening only when the
sun is fully shining
on it, and of closing soon
after midday. The
same flower opening
and closing many
days in succession.

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But the chief peculiarity lies in the Stamens which are very short and singularly convolute, forming a spiral twist around the pistil which is terminated by a very bright green stellate stigma. The flower is about two inches across, and of a cherry pink color.

As you know the ribs of the stem are clothed with short appressed spines, but the spines on the base of the tube are nearly a half inch in length.

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times in reference to any names
I may suppose inaccurate. If
which I think I have many.

Very respectfully

Thomas Meehan

Did you ever notice that when the stamens
of a fully expanded Opuntia flower are
touched they immediately rise up towards
the stigma?

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and very numerous.
I do not think I shall leave and then [illegible] this season, but if you thought it desirable I should be pleased to send you the first one I have each year.

In your collection of Cacti are now becoming daily more interesting by there being frequently rare ones blooming. Of what all likely to be of any service to you I should be happy to send you flowers of any I may have bloom. Fifteen years ago I was very much interested in the study of Cacti especially, but the

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necessity of applying myself
vigorously to business in order
to afford me leisure for my
favorite studies in future, have
not seen me the opportunities
I would like - At the sale of
Caleb Cope's Collections, rather than
see the Cacti dispersed, though
I could ill afford it then, I
bought the lot together. Since
then I have been able to add
to them somewhat, till now the
Collection embraces, over 300
species (succulents of all
kinds). If you will allow
me, I should feel it a
great privilege to draw
on your knowledge at

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Rec'd July 24th

Germantown Pa

Aug 8 Feby 9

July 12th 1859

Dear Engelmann

Dear Sir

Melocact ?
Abgeschn
Epiphyllum ?

I am very

Landsberg

much indebted to you for

your kind reply to my former

letter. My plant of C. luteus

is swelling it's seed

vessel finely, and it will

afford me much pleasure to

forward it when matured -

I do not desire the seeds

for propagation, as I have

reproduced a full quantity

from cuttings. - I am only

anxious to see it matured

as a matter of information -

If useful to you, pray

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preserve it. — Any plant
was originally given to
me by a party, a friend
of whom, I understood knew
to they brought it from
Mexico; but I will try
and ascertain positively.
I am not posted up in
the more modern discoveries
in the Cactaceæ for the reason
I gave in my last. When
I originally studied Dr.
Pfeiffer's Enumeration
was my text book,
which with occasional
stray papers in botanical

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works by Colt, Link, Otto
and others, and my own
observations, were all the
opportunities of information
I had. I am much obliged
by your references. I shall
try to procure the works.
The stips of C. tuberosus in
my plant, can scarcely be
said to surround the ovary
nor is the flower quite
terminal. My plants
showed buds as early as
February, but the flowers
were not fully expanded

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GEORGE ENGELMANN

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before June - I do not
think they are regular in
this respect. I get Echinopsis
decora, tubiflora, ~~Lepto Lyriesii~~
and some others to bloom often
every heavy watering. My
Echinocactus texensis
usually blooms in May. I
have a two year old plant
now showing buds, what
will I think be open in
August. - I send you a
flower and Mammea of
a Mammillaria I have
unnamed - marked in
my collection "Seedling"
and unlike any other one
I have.

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It is between (M. Adenana
and M. x Rhodocanthes in
my Ldt. It appears to be
one of the most [illegible] shrubs
I have, although
as my plant is but a
two year old affair, it may
be too soon to judge of its
ultimate form.

I also send you a
flower of Euphorbia
antiquorum, which strange
I have had under my
eye for many years & I
have never seen bloom
before.

I have not got
Leuchtenbergia, nor do I

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think it is cultivated in the States. The only one I think I ever saw was in the New Collection at London, where (nine years ago) they had several specimens.

When I send the fruit of C. tuberosus, I will have a list of my collection ready and send with it. I am sure

You must Myself

Thomas Meehan

I have.

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Aug Feb 22
Aug March 30

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[illegible]

[illegible] seems to me
have as strongly marked
generic characters distinct
from that to which Kindly
Referred it as any plant
can have, - The flowers more
[illegible] the point.

If you have any knowledge
of the Mexican plant, I am
very [illegible] able to name the
enclosed — Any ideas are also
[illegible]
service to you.

[illegible]

You will enclose to me the cost
of [illegible]

[illegible]

[illegible]

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MISSOURI BOTANICAL GARDEN PEABODY
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Rec'd June 4th

Ans June 12th


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——.o:——
**D. R. KING, Publisher,**
BOX 406 PHILADELPHIA P. O.



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Seminaltown ~~Philadelphia~~ May 31 1860

Dr Sir Engleman

Dear Sir,

I am sorry that I have had no more Rhipsalis to send you. My arrangements for collecting my Cactaceæ and Succulents, do not allow me to have many flowers at most seasons. Now however they are beginning to show flowers freely. and I shall be pleased to send you flowers and fruit — any that may appear the following winter & spring and,


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I open my letter to say
that I think I have
another flower and perhaps
on C. tuberosus. if so
would you rather have
the flower, or big tubers
respectfully?
Enclosed please find two
buds one open beginning
to shell.
The enclosed leaves
are opposite inflexed

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And if any of them
are desirable for
crops, and suited
for ornamental purposes
I send
Mammillaria Wildiana
" radiata cupesta
" dioica
Cereus Engelmanni
Echinopsis Decora
E. Mammillosus
Aloe vera cola
Agave Utahensis Lemaire
Yucca Rostrata
Yucca Whipplei
Yucca Baccata
Yucca Glauca
Yucca Elata
Yucca Angustifolia
Yucca Filamentosa
Yucca Recurvifolia
Yucca Gloriosa
Yucca Aloifolia
Yucca Treculeana
Yucca Rupicola
Yucca Arkansana
Yucca Macrocarpa
Yucca Brevifolia
Yucca Mohavensis
Yucca Arborescens
Yucca Draconis
Yucca Australis
Yucca Valida
Yucca Carnerosana
Yucca Thompsoniana

Jam sorry mine
two days ago. He
has already remarked
all the principal flowers
and Parisian collectors
and has half an idea
of deciding to come and
to Metz — to collect

Very truly

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Germantown
May 9th 1865

Mr. Henry Shaw

Dear Sir

I think I
accidentally omitted
to send You the Ferns
Leaf specimens in the
lot of trees I sent you.
I send them by this
days mail —

With them are a
few things for Dr. Engelmann.

Pinus pungens
Cupressus thuyoides
Pinus excelsa male flowers
" but not open yet.
" sylvestris female flowers.
Cryptomeria japonica both
cones.

These are all I think I
now have likely to be of
any service to him

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as he is studying the
cones I may mention a
curious fact which has
not been observed, - although
I no possible moment
beyond the fact that I know
of. - The left hand line of
the course of scales in a
Pine cone goes all the way
round the cone - making a
complete circuit - The
right
left hand ones go only half
round ( ) curving
to a slight difference in the
lower angles of each scale &
The beauty of the fact lies in this
that each angle line is so regular
that one could not guess whether
the growth which of course can only
be in one direction, is from left
to right or right to left by the
mere examination of the dry
cones -

Truly yours
Thomas Meehan

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Ans: May 29th
by May 17

Communicated

May 25th. 1865

Dr Engelmann

My Dear Sir

I am very much obliged
by your kind explanation in
regard to the twisting of Pine
Cones. I am very well versed
in the main principles of Morphology
but did not know exactly how
to explain the difference of
angle between the right hand
and left hand ~~Spiral~~ lines.
I understand it better by your
Note.

I have always given myself
credit for a fair power of
observation, with a tendency
to generalize on the facts, and
often myself done foundations
for laws & causes; but the
want of an early education,
and the means to procure

one when older, - having
been employed as a farden
laborer in England at
10 ℔ or 2.50 per week, till I
was 20 years of age - Gave
me no opportunity to systema-
tize my observations; or
know (much of what after
observers, had taken note
of. Once Darwin &
means state I engaged
heavily in business with the
main object of having the
means to indulge my tastes
when I provided, and
though not get quite "out
of the woods". I paid in
annually increasing leisure
and enjoyment for observing
and for study.

I am afraid to offer many
notes of what I think
are original observations,

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for fear I am occupying the time of my friends with what is already known to them, and it is only when I meet with an indulgent correspondent like yourself who is kind enough to set me right when wrong, that I venture to say much. It was only by Dr Gray's encouragement that I was induced to publish in the Proceedings of Phila. Ac. of Nat. Sciences my paper on the relative differences between allied species of Am. and European trees, and which I am informed very much interested Mr. Darwin and others interested in that branch of study. This made by way of apology for what follows about

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these pines have sent to you,
if it should not interest you.
Fructification is of course
transformed leaves: but in
the Pine family it is arrested
full growth which is formed
into male flowers, and arrested
spring growth into female
The leaves (bundles) of Pines are
but feeble shoots - the male
flowers shoots of a stronger growth
If the growing shoot is arrested
suddenly it produces instead
of leaves and flower buds, leaf
buds merely - I have pinched
off growing shoots when growing
and the fascicles of leaves as
formed be under ordinary
circumstances become buds!
You will see this naturally
in the leading shoot of Pinus
Pumilio next. It was arrested
in its growth the very near the
summit, and instead of leaves
on the afterwards produced part
they are concentrated as buds
at the base, - An earlier stage
of arrest, you will see on the same

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Most, produced female
flowers — but the presence
of Sedum Nuttallianum and
the arrested, or male female
flowers — It is now starting to
make another growth again
this season, and when it
ceases by which time it will
be fall (the arrested growth of
last season will be made by
which will unfold with the
first bursting buds of spring.
In will be seen in the
Sedum stellata, the female is
but an arrested branch —
In both cases — male and
female flowers — there is an
evident difference in the
time and condition of growth
necessary to produce the one
or the other — I feel certain
some general rule can be
detected — made plain by

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which declining plan &
can be accounted for, be-
yond getting on this threshold
I feel I have not time or
opportunity to go further.
In case the male flower
is the last stage of growth —
the maturing buds on di-
vere of the culm, although
its position on the culm is
the inverse of these on
the Pine. It is a
singular Sarcocaleceous
plant I had the opportunity
of examining this Spring
— Akebia quinata — The
flowers of which are
arranged on a sort of
raceme, the first flower
that opens & has pine
carpels, and five stamens,
the next one but four &
last, and then comes the

GARDEN
MISSOURI BOTANICAL



male flowers on the end -
In short is not a
male flower made up of
a more matured, and
(by) excelsa condition of
(converted) fronts than a female one?

I have only sent you one
female P. excelsa - I have but
few, and want to set seed;
but if any one to you the
sacrifice will be of no great
moment, and you shall be
very welcome.

You will notice Dr Engs
excelsa male from one tree
has thicker cones and of a
lighter color than those from
the other tree. They are from
seeds direct from the Himalaya.
I call your attention to this
particularly as Dr Sadler
& Hooker has recently
announced his decision

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that Mrs Pierce of
Europe and N. Accelia
of Africa are identical
species, and you may
be sure variation in
here from the same place.
I send you one piece of
Mrs Pierce's master, which
I did not mean to have in
bloom until I found it
half dry to day.
a friend in Baltimore
has begun figuring
fruiting; but they are all
now over — I did not hear in
time. If desirable he could
send you some next year.
I fear I have bored you
too much; but pray do not
accept valuable time as you
must be in replying please
here be little silent to me.
Sincerely Yours
Thomas Meehan

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Rec Sept 8
Ans Sept 13

Germantown Pa,
Sep. 4th. 1866

Dr Geo Engelmann

My dear Sir

I am much interested
in the subjects of change of
form in species, and have
made observations which
lead me to think that the
seeds of change are latent
in all plants, and have
been there from the beginning
and that by a proper
investigation we may
some day arrive at such
a knowledge of the laws
of change, as shall work
to see into the far
future what the vegetable
world will then be, —as
we can now form almost
an idea of what it looked

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* L. marginatus
L. accuminatus var debilis
L. serpuides var Macrotemon

---

We miss you very much,
Dr. Asa Gray kindly
interceded & we can
however that my views
were worth publishing
simply as a theory - but I
prefer to collect facts for a
while longer yet.

In connection with this
subject I should
very much interest me
if you would be kind enough
to name the species of
fungi which I send you
by to day's mail. *

If you should happen
to think from all one
species which of the forms
moved you regard as the
most typical?

I decided to send you
the specimens after reading

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at page 426 of your
~~Synopsis~~ Ms. Ac. St Louis
transactions, what you say
about change of leaves in
Juncus scirpoides and J.
canadensis. My idea is
that change being innate is
not so much indebted to
external circumstances as
Darwin supposes; but not
to bore you with my theories
I may say that all the specimens
I send you, are from the same
circumstances of soil and
moisture".

You say nothing of the
"Roots" of Juncii, when speaking
of "characters". They have
always interested me, and
I as much value as the
three Genera, which seemed
to me but an abortive or depressive

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which nature in her
apparently wayward moods, might
develop again at any time —
or the latent leaves, which
seemed simply undeveloped
leaves of the same species, —
& tubers being but undeveloped
or suppressed underground
runners, or simple offsets on
pedencles, though — interesting
may be of no more value
in marking out lectures:
I have seen Potatoes that
produced no tubers, but thickened
up like Raspberries, — while
Lycopus virginicus which
usually produces underground
runners, often suppresses
them into good sized tubers.
In one pincus I send you
see well formed tubers. — in
another, simply thickened fibrous
offsets, — in the third a slight
tendency to creep, with a

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Rec'd Octob 1st

Geneva Feb 26th 1866

Dear Engelmann

Many thanks for

sending up the

Specimens. Found them

Mrs. E. Smith Kat Brun

had had an attack of

Cholera, and scarcely ex-

pected an answer to my

letter so soon.

The Juncii grow altogether

in a small marsh at

Cowlesville Vineland Cumberland

Co. N. J. The next time I

go there I will bring

living Specimens and cul-

tivate them that I may

make them developement.

I shall take much

more interest in them

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I have I have done since
You decide them to be
the species I supposed you
would — as I feel nearly
sure they have all
originally sprung from
one type, and that by
watching their development
some clue to their mystery
will be evolved —

What you note of the
Rhizoma in Podophyllum
is true of many ferns and
can be particularly noted
in Botrychiums which I
suppose you owned class as
an upright Rhizome.

The fibres come out only from
the last years prolongation of
the Rhizome — all beyond
that dies —

Fully Yours
M. S. Greenman

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very little tendency to thicken tubers like *

I have similar troubles with Juncus tenuis but I am loathe to intrude on your valuable time to answer questions for me, which I ought perhaps to hunt up for myself. The greater part of my time has to be spent in the effort to make a living, and consequently the Bibliography of the Science is so neglected, that I hardly know where to look for the ~~notice~~ observations I am often inclined to think new, but most probably already on record.

If you find time without much trouble to name my pines, please refer to them as * the flat leaved, - the large one, and the small. Truly yours Thos. Meehan

THE

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THOMAS MEEHAN, Editor. W. G. P. BRINCKLOE, Publisher.

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Rec'd July 31
Ans Aug 5

Germantown Pa
July 26th 1869

Dr Geo Engelmann

Dear Sir

I am glad my
note on the Yucca interests you.
I did not know the Dr Yuccas.
Character of the Yucca head
seems well observed "Pretty
well read" botanist in this
neighborhood Still attempts to
decide whether an improved
shape is from a native or a
foreign stock, by watching for
Adventurous seedlings grown.
It is a doctrine with
intelligent Gardeners. Most

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flowers

unimpregnated, may bear not always produce edible fruit sometimes with even seeds which do not grow. I have been told, and believe it without going deeply into the matter have taken it for granted. I believe I have known Cucumbers raised easily in frames, when accidentally a female flower was come before a male is produced to produce a cucumber in Catable condition, - but at this moment I would not like to certify to it as a fact. I discussing this subject once with the late Colonel Carr and Dr Wm Darlington, who both believed it possible, the

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former said in Introduction, that
when the Osage orange fruit
flowered at Bartram the
trees were all female, but
they produced balls freely
and had seeds, but they
could not get a single
seed to germinate, — after
some years some male
plants shoots from some
of the tandem were grafted
on, after which the
seeds grew freely - and
I have myself observed that
female trees
flowering a long distance from
male trees of Osage
orange produce balls
without seeds, except
here and there one in

some of the walls. The fleshy calices of the fruit are not to be sure quite analogous to the pericarp ovaries of the grape, which I presume to bear a little on the question. I shall however watch this matter closer hereafter.

I received the package of asters last fall with much pleasure — they are all good viola — I should like the type to which mine can be referred from living plants provided specimens.

— Cereus candicans flowered with me this year for the first time I ever saw it. The large flower fruit now ripening. If you should want one for examination, write.

2

Ann: July 21 1867

I am much interested
in comparing Cactaceæ with
Portulaceæ, as they seem
to have a much nearer
relationship than Lindley's
classification would seem to

Going back to unfertilized
seeds I am reminded that
the researches of —— to
ascertain whether the seeds
of Coelebogyne Ilicifolia had
an embryo or not, seem to
indicate that they suppose it
quite possible for seeds to
be produced without impreg-
-nation, & they would not
look for evidence to account
for it - although I believe that
plant is now known to produce
Accessorial pollen grains.

I have some idea of preparing
a paper for the Proceedings to
show that Thuja Meldensis Lindl

is not a hybrid between the Red Cedar and Chinese arbor vitæ as Prof. Asa Gray suppposed, but one of many polymorphous forms the Thuja is capable of producing. During my observations on the plant I have noticed a curious circumstance:— The first growth in the spring takes a curved course due South, in a few weeks it curves over itself by another curve north, and then again back south.

I have some reluctance in publishing these papers. Passionately fond of natural history when a boy and young man, I had to abandon the pleasure to battle with disease and poverty, and during the last three or four years that I have been able to take up the broken thread, I feel that I am so liable to write as new, what is most likely already known. It is only that I feel our proceedings have so little of botanical interest in them that I venture to offer anything at all..

Truly Yours
Thos. Meehan

Enclose sketch of Thuja seedlings

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Rec'd July 31 1867

P. S.

I thought you would like some Vitis Cordifolia fruit mixed & dried. I send some to-day. They are from two plants growing within a few feet of each other in a waste fence row. ~~I have~~ one had a large quantity of racemes without a berry as you will see. The other have some berries but many imperfect ones - I think the fox grape never sets all its flowers.

I think you will observe ~~the time of~~ the leaves have lights blotches through them, which perhaps if you place under a strong microscope may prove a parasite in process of development

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Rec'd Aug 26

Germantown Pa
Aug 2 22 1867

Dr Geo Engelmann

My dear Sir

Anxiety for my
children through Severe Scarlet
Fever kept me in the house
until the end of the Cereus
Candicans bloom and dried.
but I will keep the remains
to St Louis, where I hope to be
during the Pomological Convention.
I leave on Monday next for
the west. The Cereus is a
Nocturnal flowerer, The Petals
are short in comparison with
the length of the tube, with the
stamens as long as the petals.
The fruit is depressed globose,
covering (here) of an orange
brown color.

I enclose you some seeds
of Picea balsamea Pattheri

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to day from two different trees. The purple headed one is the form I have always seen and considered normal, — the white headed form I have never seen before — on removing external cuticle to the seed paper in will see that it is quite unlike the usual Fir Seed, and approaches very much the form of the Norway Spruce — I send you a few Norway Spruce for comparison — There are many scores of Balsam Firs, and Norway Spruces on these grounds, some of which are entirely male and always produce nothing but male aments, — some only produce a few female — some many female, — but all I believe

have more or less males, whether they have females or not.
This year only two trees ~~matured~~ produced any female cones ~~last~~, I do not know whether they produced female ones or not in the spring but it is possible that the tree bearing these pale seeds has been fertilized by Norway Spruce pollen — a late frost or some other cause destroying its own?
I have never known the Balm of Gilead mature cones without fertilization — that is I have not observed whether they do or not, — but the Norway Spruce does, which I had forgotten till now, — another instance to add to yours of the Pine apple. These seeds are never found in these cones.

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The observations on Thuja à Medeus are on 25 plants growing side by side on my grounds. All have the same exposure since the first of August the Curves have commenced straightening from the bottom upwards, and the "kinks" have now nearly disappeared except the tops which to day are all curved towards the north except two plants, which have not entirely, and are now falling upperfectly upright — I think I can account for the ~~death~~ distortions growth of the arbor vitæ, by this peculiar behaviour of the plants, but am waiting for further observations.

Very truly yours

Thomas Meehan

Germantown Pa.

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Rec Dec 30th
Ans Jan 7

Germantown Pa
Dec 24th 1869

Dr. Geo Engelmann

My dear Sir

I was last week in Ottawa Illinois, and I gathered there two very distinct varieties of Opuntia vulgaris, which I now send you - The round Rectum I found on the "Buffalo Rocks," about one hundred feet above the Ill. River, — the ovate form and long fruit, is from the farm of Mr. Ned Bass near the Mathematician, who complains that the Opuntia makes unprofitable resting for his cows in Summer. — his pastures are full of it. The round one is very nearly like the one I got three years ago on Mesa Bluff above your town, and which you saw in my Garden. The other is like the common one you saw also here, only for its Spines - enormous thorns, and very long fruit. I have many things I would like to say to you but am sure you must have enough to do to catch up with many things which have awaited your long absence, & attention.

Very truly your
Ned Sheehan



I cannot help adding that I have a very interesting Pine specimen sent me by Mr Ravenel who found it after reading my paper on conifers leaves.

The scales have developed into "foliaceous sacs", and are here serrate as I described the leaves of the young tree



This gives the idea very roughly. I shall have a few remarks on it for the academy next Tuesday, and ask them to suspend it in the proceedings.

Mr Lapham of Milwaukee was with me and promised to send the Opuntias, but for fear he forgot, I send.

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MISSOURI BOTA
GEORGE ENGELM

Rec'd Jan 18 16
Ans Jan 19

Germantown Pa
Jany 14th 1870

My dear Sir.

I notice Mr Candolle has quite recently endorsed Brown's theory of the structure of coniferæ flowers. It cannot be correct if my theory of Pinus leaves is the true one. Has it ever been demonstrated by actual microscopical observation that the ovule is produced from the upper surface of the scale in Pinus? So far as I can understand by cutting open a large number of cones, they proceed from the cellular matter underneath the scales, and the mark on the scale beneath, on which the seeds appear to lie, is but the result of pressure. This cellular matter (bracteal) is the analogue of what I call true leaves, and although I doubt whether the leaf has any developing influence on a bud, yet it is usual for them to appear in connection. The scale being but a metamorphosed branch and not a leaf, it is unlikely that a

seed would be found above and in their axils. My observations on split cones, renders this almost certain, but I cannot trace the matter up so close as to be positive. I thought you might know whether. Pere ovules had ever been noticed by the naturalist else in any certain position.

again is it certain that the the female flowers of our american Pines are fertilized the first year? By noting the direction of the woody fibre in these cones, there seems to have been not only a remarkable cohesion of parts in infancy, but a curious doubling up or inversion, as I looked out for you in the "stipules" of magnolia, and if this is so, it will account perhaps for the anatropous ovules. In cutting open these Cones, I notice in some species ~~the~~ there is a large oil cell at the base of the scale, in some it is such clear, in others pearly than



those Pencloses. In some such
been at all; but always when
present in the same Cases. It
is curious that on the back of
the true leaves of Ferns often is a
a Similar Gland, and that the
seeds of Pines come out (as I
suspect) pair about the same
position on the bracles,
Can there be any relations ship
between these & cells and
the ovary? (for I half think there
is an ovary in Pile of Borum)
I suppose you have seen the
oil cells on the seed of Picea.
It is difficult to clean the turpen-
-tine out to see them nicely, but
if the few I happen to have by
me I send you one, for I never
met any one who has seen them
but me, but you have studied
them so much closer than I
have. It always seemed to me
I cued divide Abies from Picea
Abies by the seeds alone, but

only from there or the cells,
but from the back of the leaf
being surrounded by a
Caspellary scale, while in
others there is a scale only
on one surface —
many many times
more numerous

I Came Down from camp back
last week ——

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about Noon hungry — usually
it has two "leaves" — But we
were speaking about the Waves
Waves of growth, each weaker
than the first of the season. I find
that the last wave produces
three leaved fascicles. Has axial
origin anything to do with number?

I have tried so often to tear off a scale
so that the lead attachment should not pull off
and have never for the first time succeeded,
I send it to you, but in trying it came off. The
union is by the two legs of the "Caspellary"
scale to the two little protuberances of the
transformed leaf.

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Rec'd March 11
Ans Octo 15th

Germantown Pa Feby 26/70

Dr. L. Engelmann

My dear Sir

I was very much interested in your suggestions on ~~Vines~~ the influence of Pines. It seems to me that some facts that I have observed are still inconsistent with the explanations; but I want to examine that further this season.

What I meant to send you now is a singular fact in the duplication of buds, which as you know I have been interested in so long. While with my friend Joseph Hooper yesterday, I noticed on some very vigorous young Magnolia acuminata shoots, that there was a lateral bud uniformly on the right side of the one in the axil.

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Examining my plants at home, I could see no lateral bud, but on examining the wood I found that there is one under the bark. As fast as the tree grows the lateral one grows so as to keep always under the cuticle (pretty much as I pointed out to you the buds of Gymnocladus do). I also once examined many other trees to see if others had similar buds, and I find on many Leguminosae & other Wood of Celtis, that there are a pair of laterals, distinct from the central bud, going along together and only becoming apparent as the branch increases in age. I thought you would like to see specimens. I suppose you will see as I do, that these facts do not help the usual distinctions between axillary and adventitious buds.

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Moreover I have never been satisfied with Bentall's figures or Priddey's explanation of the formation of Bark. I never could see why it was such as necessary for a Beech to "crack it cuticle" with Spruce, as an oak or Elm. Nor why "cracking of the cuticle" should produce thin broad scales in the Beech like Liriodendron, and true and beautiful cork in the allied Aesculus Glabra and so on - and I have thoroughly suspected that all this bark exfoliation is the result of an peculiarity a specific tendency vitality, or the production of leaves or stems. But I can find nothing in any book, which hints at this. Can you direct me to any treatises on Bark formation that would interest me?

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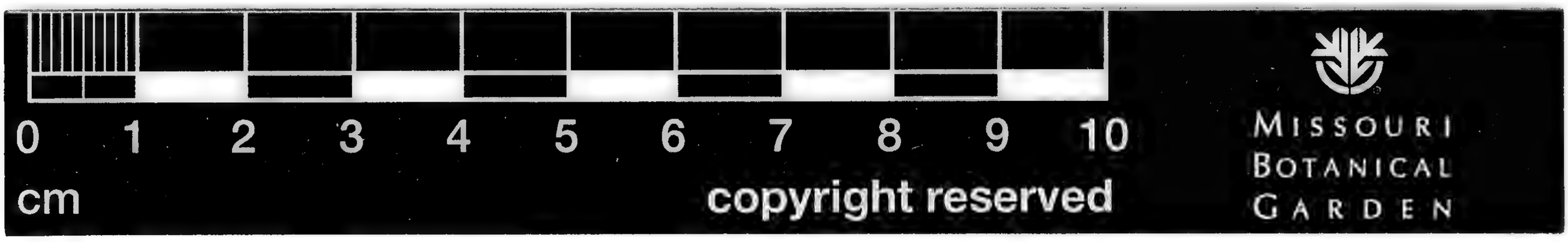
The true plan of exfoliation is very clearly set forth in Sassafras. Diamond Shaped cork cells are formed on the young shoots. In two year old, Petiolar elongate, and cork fibres are developed, under the outer pellicle, which is destroyed in their development. Meanwhile tangential touch latch the first year, while the cork fibres thicken, & new ones to add density by numbers, and by the fourth year long rows of cork by radial division, which make a new layer, underneath the other every year. Sometimes this cork development kills the bark, almost to the wood, but when these cork cells are not developed the bark lives as long as the tree lasts — not a particle dies each year or any year. A layer of liber is made every year, which

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never changes its nature to become wood as some think, but it can be counted in the bark of oak and chestnut and other trees, after it has been one year, and the exact number of layers tally exactly with the number of concentric rings found, except when destroyed by the development of cork cells. Cork cells develop usually when the tree is from 15 to 25 years old, although on white pine and some other trees they have been as once

This seems to me to be true, and I have wondered for years how it can be true if a new layer of bark is formed each year. A chestnut for instance certainly never loses a particle of its bark until it is twenty years

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old. Then it will be about
~~twenty~~ ~~two~~ thirty feet in
circumference. There
ought to be a crack in
the bark somewhere before
this if the ~~supposition~~ theory
adopted therein are true. It
seems to me to prevent this
cracking there should be
a lateral additional every year
to each annual layer, as
well as the production new
matter of a new one -
But your rule pay logic
has nothing to do with
physiology - "We must have
the facts", and then I have
not yet, only - I am not dealing
just with my early teaching
"Trusting you will have
patience with me I am
very truly yours
Jno. Mellon

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Rec'd April 9th
(Ans Octob 15—

Germantown Pa
April 4th 1870

Dr. Engelmann

My dear Sir
when here I gave you a
specimen of a true Osage
orange. You have found
many of them this season -
In one case ~~known~~ either
one of the trees has either
died out (as ascending axis)
or it has become thoroughly
incorporated with the
other stem.

I thought you
would like to have it as
a companion to the one
you have -

With kind regards
Yours
Thos? Meehan

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GEORGE ENGELMANN PAPERS

Rec'd July 13
Ans'd Oct 6 15

Germantown Phil. July 8th 1870

Dear Dr. Engelmann

You know when you were here you opened my eyes to an exception to what seemed to be universal rule, namely that the lower end in the longitudinal series was the largest in Sycosteum. I have since seen that this is in all Cucurbitaceæ and Cucumiferæ I have examined. Also I have since found that these longitudinal series are to be found in almost all plants nearly plants at times but so far I have found nine like these Cucurbitaceous plants. It has puzzled me so little and I have been waking from continually to try to account for it.

I have now found that in the principle of their formation they are probably no exception. & the upper or large end of the ovary produces the flower when the two ends are present. So also do the

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for want of the direct
evidence, and I have to wait
but I do say "Nature's Root"
bore other one, than proved
what I suggested to you; that
Pinus could not be a Syncarp
Genus

Also. I note that
Dr. Spruce and others are
working to take the credit
of the theory I set, which
I advanced last year —
namely that it is not the
highest efforts of vitality which
produce the male sex in
Asp. Thuja reflexa, but the
reverse. Spruce says
he came to the conclusion
a 25 years' ago, and "wished
soon to publish a paper" to
prove it. But his Cascade
Mts. Cactus!

Yours
Asa Gray

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upon large buds of Halesia tetraptera - in bowiana it is still the large buds which flower, though the shoot is renewed. I think I shall find out some interesting law in reference to these matters somedays, and keep on making observations with a great deal of interest.

I satisfied myself that I was right about the early unfoldment of Spirea. but could not get about the muscle-wood. But the hearts are so small - and I can only argue that if I am right in my views of the morphology the principle of construction prevailing therein of course structure must be wrong; therefore I can make no progress in the certainty of my point

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Rec'd L 29
Ans March 16 1871

Germantown Phila Oct. 26 1870

Dear Dr Engelmann

What a beautiful cactus!
I shall do my best to raise it. Do write
the account. But are you sure about the
hybridization? In my own experience
I have lost faith in them, and believe
they rarely occur in nature if at all.
It seems the normal condition of our
Atlantic coast to have lobed leaves,
and the entire leaved vine seems
occasionally to resume, or attempt to
resume, a character they have lost.
Dr Gray gives me credit in the Manual
for discovering a plant of 2. heterophylla
in Delaware; but I never considered it
more than an accidental thing — just as
the paper mulberry will do when its
powers are not strong enough to develop
all its veins on which the more entire
leaf depend.

After you were here and my
Sprattia ripened their fruit, I found
that the seeds of the two were identical,
and remembering what you said on
this subject I was puzzled. I sent
a man some years ago to New Jersey to
gather the seeds, and he brought this
Sprattia back. I understood it was wild,
possibly from an old garden. I will try
and find an undoubted New Jersey locale.

This longitudinal bud subject is prov-
ing more interesting every day. I now
find to a certainty that the appearance
of the peduncle in (many Solanaceae be-
tween the nodes, is a case of this character

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The peduncle is merely from one upper bud in a series of two which belongs to the node below. In the Cherry Tomato it is best seen, as it sometimes is found almost down to the lower node, and even when nearly up to the upper, if torn back, the fibres can be traced back to this node. This observation led me to note that the peduncle in all Solanaceae that I have been able to examine proceeds from every third node, and that the axillary buds




are numerically marked: 1 being leaf has scarcely a bud visible; 2 shows axillary bud; 3 very strong, pushing into a secondary shoot in fact, and above this the extra or supernumerary, producing the fruit. In some Solanaceae the nodes get bifurcated; in Cyclamen radicans




and 2 being close together. In Datura Stramonium all three together, and the peduncle bursts out between them although evidently the fourth production of the dense shoot. Now the interest of all this is that the same phenomena exists in Phytolacca. except that I have not found the peduncle so variable in position. It extends sometimes a 1/4 inch above the node opposite, but I have never found it between this upper node as in Solanaceae. Is there really much difference, between after leaving out the addition of stamens with the axillaries, between Phytolaccaceae and Solanaceae, except such as may arise

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3/ from abortion or Multiplication of parts? I made the suggestion last summer to Dr Gray; but he replied "how absurd! It shows you know nothing of the principles of botanical classification". Whereat I was for a time frightened. But I keep hacking away at every specimen I get, and sometimes in spite of my desire to accord with Dr Gray's views, I cannot get the idea out of my head. To day for instance I cut across a half ripe berry of Solanum capsicastrum, and find it to be called; and the pedicle of Datura meteloides has a bundle of fibrous tissue in lieu of that Dr Gray's character in Manual, "fruit 2-3 rarely 5 celled" is not absolute - To day 5 and even 6 flowers. The Datura often has 10-15- and once I found 25- although still in the 5 set.

Following out the hint you gave me last year about grape vine tendrils I find ampelopsis almost always has its third node without a tendril; and the two with tendrils without axillary buds. There seems something of the same before noted in Solanum: every one the tendril of a grape vine belongs to the node below? It seems scarcely possible; yet how can it in physiology bind itself directly

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4/ above the lower axil, as in the Solanaceæ? It does not seem right, and yet I see no variation from exactly opposite the leaf, and at the node, as I do in Physalis and Datura.

You suggest a change of position sometimes in the odd kind of phyllotaxis - I found it was so that seen in Bocconia cytindrica - You know that the weak peduncle which bears the male flower comes out at the side of the ~~one~~ stem one which bears the female. This is usually on the left, & upper side (if I remember right) of the plant.







This continues on node, when the two bear the male peduncle on the left the other on the right. So that they face each other




1, 2 males.

In relation to sex, and the term vitality, I have often thought of your objection indeed it has often been made. But I have no other word that comes near to what I mean. I mean by vitality simple life and I mean by a high grade of vitality

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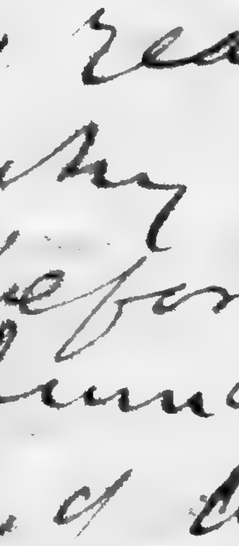
a condition far removed from death
which in my vitometer I named
call zero. — When plants are
growing vigorously, I do not take it that
they possess so much vitality as when
they have pass this stage and commence
to bear, — even if the reverse of this
were true, it would make no
difference to my theory of sex for that
deals only with the conditions of
vitality at the reproductive age.
If the growth as you say stunted always
continue "so stong" as never to bear
fruit, simply then my theory would
be inoperative. It would not be a
question of whether the plant stunted bear
a male or female, but whether it stunted
bear at all. But when it did com-
mence to bear I take it that whether it
produced male or female flowers
abundantly would depend on whether
the sources of nutrition were turned
into the channels of stamen buds which
produced flowers, — or the degree of
nutrition which flowed into the
buds. If full nutrition went to a
certain bud it would be female, — if
impaired nutrition male.

We always revive Cacti by decapitation
when they do not sucker.

You say the "law of expenditure in
bark — there is not the diligent degree

6

of expansion. This I have tested. If you put a piece of tin or wire closely around a tree so that the edges meet at - as the tree grows it will push open at a but will leave as if the ring was united continuously - also if the bark - the external cortical layer (as well as the liber - in the common elastic does not stick till the twenty fifth year - in the Beech it never dies. why has not expansion split this bark before this time? In the Maluaceæ, Lauraceæ, and some others, a patch of bark (a) will peel off this year, another patch (B) peel off next year, yet all be of the same age




My idea is that bark dies by the development of cork cells - perhaps a sort of pseudo fungi - not by expansion; or necessarily by age, - and I was anxious to know whether any such an idea had existed, - or anything of the nature consequence in the true nature of cork cells had been published

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If nothing of this has been published I thought to put together a series of observations on various heads, with the heads of their individual Varieties, and some short facts, for the amusement & association next Year.

I am a little hurried just now with [illegible] business, but thought best to write at once before the Frosts suggested by your kind letter caused [illegible] my visit.

Yours sincerely

Jno. Jackson

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Rec Nov 28 1870
Ans March 16 1871

Germantown Phila Co. Nov. 25/70

My dear Dr Engelmann

A severe trouble with diphtheria in my family, in which I lost one of my daughters, and others scarcely rest out of danger, has led me to forget until to day that three weeks ago through the kindness of our mutual friend Mr Burk, I got a piece of the Opuntia from its native locality Woodbury New Jersey, where I supposed, but did not like to say positively mine originally came from - Mr B. says there is at least an acre there, and he is sure no escape primary garden. Still I should suppose it must have been introduced there some time within the white man's advent, or a plant which is so well able to maintain itself against most all enemies, ought to occupy more territory in thousands of years. Still as Jersey has been mostly one vast pine forest, and I suppose the Opuntia would not live in such dense shade, it may have been confined to a 'circle' set by the shade enemy. Certainly as you say, the characters agree with your

GEORGE ENGELMANN

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Fig. 9 and description of O. Rafinesquei in the Boundary report, and that with O. Vulgare, and I am very much interested in knowing what the real O. vulgare is. I think more than likely that these plants vary very much from seed without any hybridization, — as we find it in other allied plants (the Sonchus for instance) and that they are all forms of one thing. I think these plants very rarely increase from seed in their own places of maturing them. The young plants probably being crowded out by the old ones. In my own Rockery where there has been every chance by the annual ripening and decaying of the fruit, I have never noticed a self sown seedling plant. On the other hand if a head or fruit get carried away some distance by bird, animal, or any external agency, and get to grow, varying a little from the original a new centre of another variety is formed -

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Lo. Nov 28 1870

Thus on the farm of Mr. Barnet at Ottawa Illinois the large balsts had all immediate erect pods, — while a mile or so away, on the Buffalo bluffs overhanging the Illinois river, they had many round pods. So on Piasa bluffs above alton (I saw only a few scattered plants here to be sure). The few plants I saw had no trailing tendency, but the pods were set one above another in an erect position, and the number of small white spines was most striking. Yet the little piece I stuffed here is now a bush of a foot or more over, and has no erect tendency, though retaining its spines. In this case probably the erect habit was through growing amongst undergrowth. The two little pieces brought from the Illinois River and Ottawa, have also branched the last year, so that I can send specimens from all three localities if you wish. although now three summers since I had the Piasa one, it has shown no disposition to flower yet.

I have a pretty mammillaria from Colorado. It was only about ¼ inch high when I set it last year. It is now about 3 inches by one broad, perfectly columnar. Skin dark green. Mammæ

tapering - the lower series of numerous (5 to 8) bristles white - the longer (5 to 8) awlled somewhat recurved - Probably it will flower next year.

Very sincerely yours

Thomas Meehan

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# Germantown Nurseries,

THOMAS MEEHAN,
Proprietor,
Office, 5547 Germantown Avenue,
PHILADELPHIA.

Germantown, June 20.

Dr Engelmann

Recd Aug 16
Found Aug 15

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I have been so
of our Academy, and no
but with home matters
have decided to devote
themselves to a great work
of which the enclosed Specimen
Flora one is already known fa
vorably as a successful li
brarian and artist, with a
very large business. He
often has known for what th
has done in Conchology.
I also a fair botanist with a
growing love for it. They a
young enough perhaps to see
the end of it. But I hardly
expect to, with I have decid
to aid them all I can in ca
out any labor of research.

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# Germantown Nurseries,

THOMAS MEEHAN,
Proprietor,
Office, 5547 Germantown Avenue,
PHILADELPHIA.

Germantown, .....

If you feel interested in
a word or two of encou
ragement in such an en
terprise, they will highly
appreciate it.

Very truly yours
Thomas Meeh

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THOMAS MEEHAN,
Proprietor,
Office, 6347 Germantown Avenue,
PHILADELPHIA.

~~22~~ June 27
~~July 4~~

Germantown, June 24 1871

My Dear Dr. Engelmann

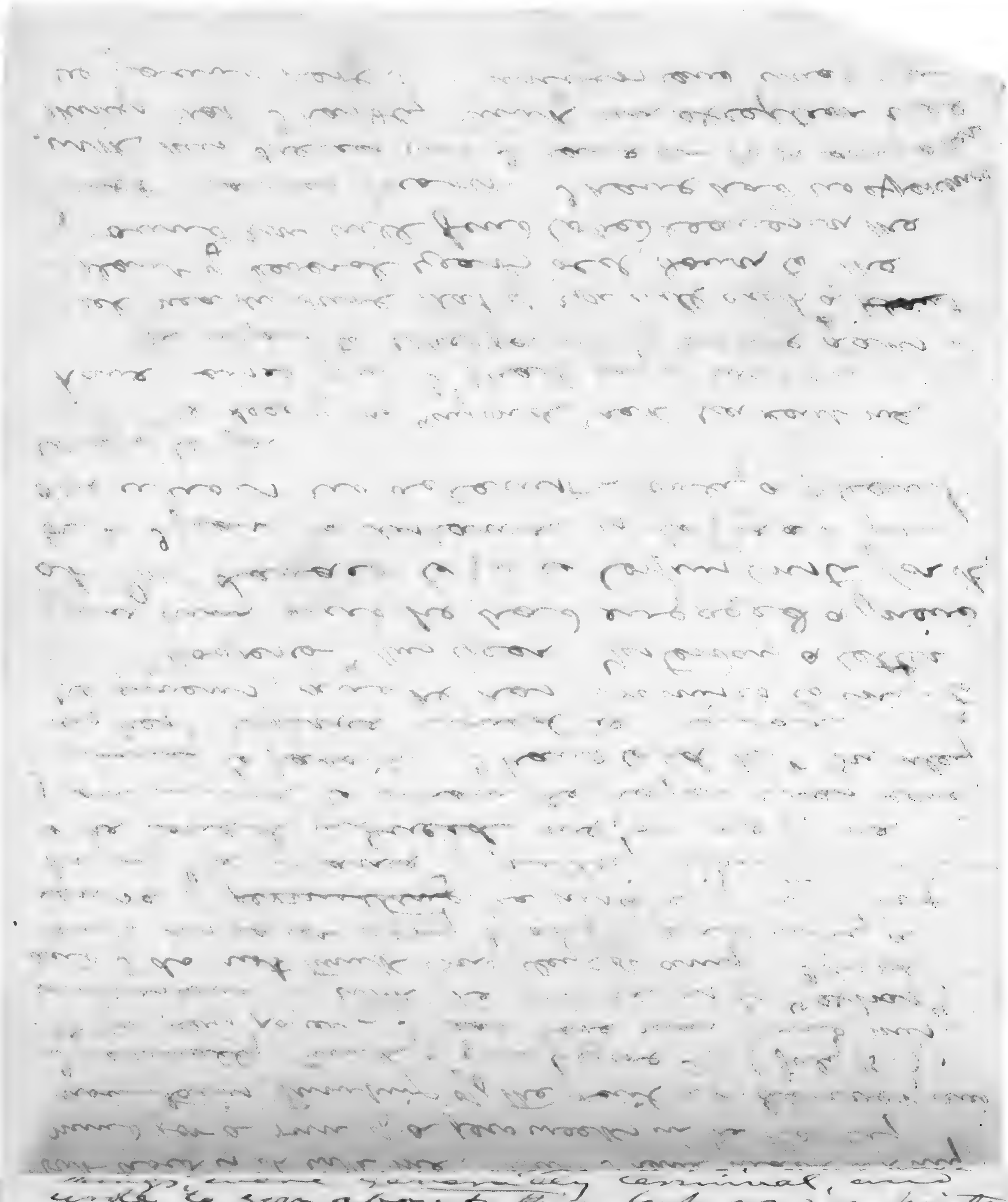
You say you are a bad correspondent, but how is it with me! But I have made up my mind for a run of a few weeks in the Rocky mountains, limiting it the rail at Denver, and I naturally think I will see before I go (July 15th). 1st Buckley's paper - I fear there was some misunderstanding with the printer as to "extras" and I do not think Buckley got any. I do not think his paper very strong, and only a sense of ~~permitting~~ the justice of affording him the chance of putting himself right if he could induced me, as one of the committee to whom the paper was referred, induced to pass it. I have told Mr Buckley his best defence would be new and better specimens, and he has promised to try to get "Hesperis" this year. Yesterday a letter from him said he had engaged a guide at Port Lavaca to go to Corpus Christi for it, but I fear the distance is too great for one who is no botanist - only a plant lover to go.

The seed of the "shy bird" oak you sent me, I have raised, and I shall enjoy watching it.

In regard to lobed forms of entire oaks, I feel nearly sure, that if you will cut a plant of several years old, down to the ground you will find lobed leaves on the next seasons shoots. I have had no experience with this species, but I have or so many other things, that I hardly think an exception like be found here. I Understand what you

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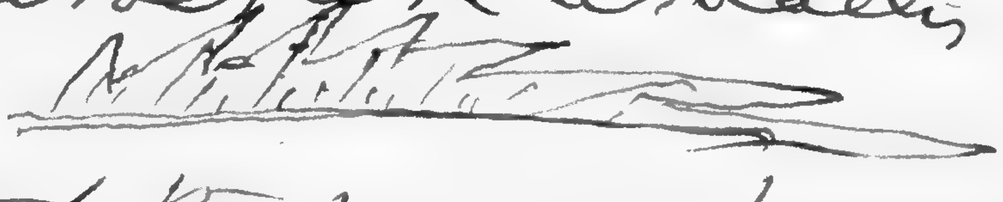


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mean when you say that the same laws of Sexing that would apply to morus or vitis would not apply to the oak. That is primarily, but yet in a secondary sense, are they not the same? It seems just as in my laws of Sex. I noted for a long time that the male flowers in ~~Cyp~~ Carex and other things, were generally terminal, and wrote to you about this, but as you pointed out in reply it was not so in many sp. of Pinus &c. It seemed as if there were varying laws, but really, as I saw at last, it was all one uniform law — the female was always in the lines of the most perfect nutrition. So in this matter of Sexing: when the flow of nutrition is regular, and each vein gets its proper supply the outline is regular, but let these courses become obstructed and they cannot develop, - Sexings follow. The cut leaved Beech or Chestnut shows this very well, as the nerves are usually very regular





You see in the latter that the nerves have not developed and that thus the "cut" leaf follows. In the Solid veined Honeysuckle, you can also see it very prettily illustrated. Will you say that this non-development of nerve is because the ~~leaf~~ blade in matter is not developed? As Agassiz says or did say, if any case of Sex the male flowers (on monoicous plants) make

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the weak branches, and not the weak branches make the male flowers! But, in the lobed leaf, I think it more reasonable to assume, if we must assume one way or the other, that the skeleton must be sketched out before the flesh which covers it.

I received the specimen from Dr. Schott from E. Simpson, is now 1/4 larger than it was, but no signs of flower this season. are you sure that the O. vulgaris in Italy is not O. Raffineseni also. I knew O. vulgaris in England as it was called there, and my memory of it as this O. Raf. O. vulgaris, as Dr. Schott sends me, so far as one may judge from a frond, I never saw. Yet of course I will not be certain after a lapse of memory over 25 years.

I know that the appearance of stamps in Solanaceæ is called stamps, but I did not suppose my explanation of it was original. and if I understand you now you have not my idea of it. as I understand pendulous & recandescence which I think is what you mean, a flower shoot from an axillary bud, has partially developed, but has become adnate with the main stem [illegible] or in the Cactaceæ. now my idea is that it is simply a case of supra axillary buds, as we were looking at when you were here. Thus in Gymnocalycium 21.00 they are never constant

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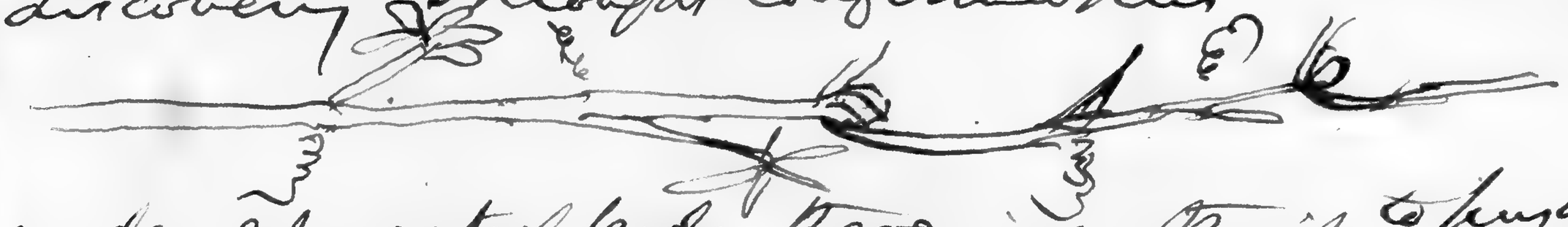
in relative distance. Sometimes the upper
bud
large ~~node~~ is almost opposite the next
node above. If you look at the common
egg plant we see with the same thing, when
young before much developed two axillary
buds. Sometimes the upper (uppermost) is
almost close to the lower axillary bud, at
other times almost up to the upper node.
Where is the "adnation of the secondary to the
primary & is "here?" Where is the necessity
for it? And such "adnating" there? The whole seems
simple and plain, and thus you see
we need no special law for Solanaceæ,
but we take the flower of Nicotiana in
Nicotiana, and the tendril in the vine,
all alike as developments of an
upper bud in a set of supra axillaries.
I have found in Rose supra axillaries that they
are not organized as the lower ones are.
They are generally the larger, and they
produce the flowers. Though in Xylosteum
the lower is the larger, yet it produces the
flowers.

My idea was that not only in Dipsaceæ, but in
all plants, the leaf was terminal, that the bud at its
base was a new development, and that it was the
development of this bud, which forced the leaf out of the
perpendicular direction. I could never
account for the zigzag growth of most young
shoots in any other way. In some cases I

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Virginia, Alabama. By May remain terminal at the conclusion of the growth for the year. My hypothesis discovery brought confirmed thus



no development of buds there is nothing to push out of line either preceding node or leaf — Axial development alone (a bud of course originally) as by his leaf out a little, but forms straight on itself, look at the young Zizyphus growth of Celtis! How is this accounted for unless the leaf is terminal?

Then your "Laws of Phyllotaxis" are not regular, even in the same ~~individual~~ individual to ~~say nothing~~ of species. The very vigorous upright shoots rarely follow the same numerical arrangement as the weaker ones. This even in the Elm and Linden which you refer me to. But perhaps the handiest illustration of this are in Symphoria racemosa. In Palinurus and Zizyphus it is very striking, and as Ravenel tells me, but I have not myself seen, that in some plants even the growing spiral turn is sometimes to the right and sometimes to the left, in the same individual.

I am sure the fissures in bark are the results of the development of subcorous cells. I have now no doubt about this.

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shall not be at the meeting of an assn. at

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On account of going to Colorado, I fear I shall not be at the meeting of am. assn. at Indianapolis; but I have sent them a paper "On the monocotyledons, the primary type of all seeds" some time ago I sent specimens of my cotyledons to Dr. Maxwell Masters, and he writes so singularly pleased with their novelty and interest, that I have been moved to generalize a little on them in that paper. It would seem that the division into cotyledons is but an accident, and, in the majority of cases, of no more benefit to the plant than if the whole matter had remained in one mass. I think I told you once I had found that the scales of Selenia were double and inverted, and that the seed was only apparently in the upper side, but really in the lower, or in terms the ovary and, but if not I intended to do so. I see by a note in Gillman's journal recently Dr. Gray notices that this was discovered in Germany last year. The other part of his note - relating to dicotyledons - I thought might have been more properly credited to Bot. Am. Assn. Chicago meeting.

My Abies Leptolepis is having cones this year.

I mail a piece of the Opuntia from my garden, but I am sure it is

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originally from the same locality as that seed — from Mt. Buck. Among some dried specimens rec'd to day from Lat. 40 in Colorado, I note a pretty Echinocactus — comison — about two inches long — The asters you name I have growing for you — I will send you any living asters or Solidago I meet with. It is not much trouble to take up any I see — I have not been able to get the Opuntis nigida for Ravenel — (in fact have been sick, several days confined, and besides hard worked) but will do it next week. O. nigida here, opens its cones every year. I have noted that under culture Opuntias, is about ten years, in its juvenile — cent condition before it grows freely.

There may be some error about ~~California~~ cones of O. nigida, and others remaining ~~closed~~ closed for several years. O. fragilis, &c. ripens and others open by drought, a fact (the same thing), and close again when it gets moist.

But though I have much more I would like to say, you will never get through to long a letter. Ever truly yrs

Asa Gray

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GEORGE ENGELMANN PAPERS

Our party will go through St Louis
and I hope they may stay long
enough to let me see you -
What is the number of your
residence?

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# Germantown Nurseries,

THOMAS MEEHAN,
Proprietor,
Office, 5547 Germantown Avenue,
PHILADELPHIA.

Rec'd Sept 18
Ans " 21

Germantown, 12 Sep 1871

Dear Dr Engelmann

I enclose you a rough draft of a paper I propose to offer to the academy nat. Science and should be obliged if you will give me your opinion about it. I have examined the male catkins of Corylus Americana, C. Avellana, C. rostrata, & C. rostrata and have found the latter scale while the former scale while I was in the Rocky mountains. Can I be sure these specimens herewith send you, answer. Hence they were not pressed. I add C. Americana gathered here to day.

Last week I was in Richmond Va, and was struck in riding on the R. R. with

The aspect of what I suppose is Pinus ponderosa. I could see no difference in their appearance between them and the P. ponderosa of the Rocky mountains. The leaves were longer, more flaccid, bluer green and the cones (large) were longer, and singly — never in clusters. I had no opportunity to handle them —

Ever truly yours

Asa Gray

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# Germantown Nurseries,

THOMAS MEEHAN,
Proprietor,
Office, 5547 Germantown Avenue,
PHILADELPHIA.

Rec'd Dec 11th
Ans — 14

Germantown, Dec. 7th 1875

Dear Dr. Engelmann.

I have been at work the last three years getting the scattered collections in the academy distributed throughout in their proper places in the general herbarium, and ~~adding~~ adding to the index the genera new to the collection. I am anxious to be able to report to the academy that this is completed, hence I have given every spare moment to the academy determined not even to go over the morning of my Rocky Mt. plants till this is done. I have them placed in separate order sheets, this is all. I found only two Arcenthobiums, and I send you specimens of them with this. The host one is from Arizona pur- -chased. The tree I gathered it

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from, leaned forward & shouted
with heavy heaving Specimens
several days afterwards - per-
haps 70 miles from the other
near the entrance to the South
Park I saw another which
I took to be a ~~was~~ another
Species, or Pinus contorta. We
had no time to examine, only
to "grab" anything we saw
and run for the wagons, which
went continuously without
a halt, except at camp times,
but I cut off some - a speci-
men to day, and concluded it
was but a male form of the
other, until I saw one branch
with a few berries on, which I
add to send. We were hurried as
I said - there might have been
more but that was the only one
I could find in perhaps 3 miles

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**PHILADELPHIA.**

*Germantown,* ..... 187

Research. Of the leaves we
I have several pieces, but the
one on the branch with fruit is
that only one I responded to.

It is certainly very
strange about the relationship
of that Hagenbuchia Cryptus to
C. Aullana — I have written
to my correspondent for further
particulars about the extent
to which it is distributed,
and will let you know
when I know.

Ever truly Yours

Thomas Meehan

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# Germantown Nurseries,

THOMAS MEEHAN,
Proprietor,
Office, 5547 Germantown Avenue,
PHILADELPHIA.

Rec - Dec 21

Germantown, Dec 16 1871

Dear Dr. Engelmann

Here are doing far more good with the accounts bills than I possibly could. So if you prefer to keep the Specimen of A. am. or Pinus contorta do by all means — You need send none of them back. A piece of the New York one would do just as well, and if you have two pieces to spare I may present one to the academy & Herbarium to match the better, — or better still if after you have finished studying the genus you could send a whole set for the academy.

Yours truly
Thomas Meehan

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